

Interactive comment on “A Weekly Arctic Sea-Ice Thickness Data Record from merged CryoSat-2 and SMOS Satellite Data” by Robert Ricker et al.

Anonymous Referee #1

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General comments:

In this manuscript the authors utilise the complementary sea ice thickness retrieval characteristics of SMOS and CryoSat-2 to produce a weekly merged thickness product. SMOS and CryoSat-2 sea ice thickness results have been published previously, hence this study concentrates on merging the thickness products and evaluating the result. The provision of weekly data, which are optimised for all ice thicknesses, will provide further insight into how different Arctic sea ice classes are changing and potentially benefit modelling studies. The method is, for the most part, rigorous and sound and the authors should be credited for this. However, I have major issues with a.) the structure of the paper and b.) the level of information provided regarding the limitations of each retrieval method and their associated uncertainties. The latter is particularly crucial, as it forms the justification for the body of work presented in the manuscript

C1

The introduction is erratic and contains a wealth of information that would be better placed in Section 2 – Data and Methods. The introduction should be just that – an introduction only – and should summarise the importance of sea ice observations, introduce the SMOS and CryoSat-2 missions and the idea that they are complementary (due to their method and relative uncertainties which will be expanded on later), and introduce the body of the paper. However, uncertainty maps (Figure 1) should not be included before the development of each product and associated error budgets have been described.

Although the authors introduce the CryoSat-2 and SMOS thickness retrievals in Section 2, they do not efficiently and clearly communicate why CryoSat-2 retrievals are reliable over thicker, MYI but not over thinner, FYI, and why the opposite is true for SMOS retrievals. In addition, a full description is needed as to why CryoSat-2 and SMOS uncertainties are complementary – it is not sufficient justification for the work presented to just state that they are/show maps. Some attempt at justification is made in the introduction, but it lacks structure and needs to be significantly expanded. I suggest streamlining the introduction then dividing Section 2.1.1 and 2.1.2 into sub-sections, or something similar. The sub-sections could be arranged as follows:

First sub-section (2.1.1.1 and 2.1.2.1) – retrieval description

- * Outline the retrieval method for CryoSat-2/SMOS
- * Describe theoretical limitations of the sensor for thin/thick ice retrievals, with a clear explanation of the reasons behind the limitations
- * Quantitative stating of thickness limits, or range of (if condition dependent)

Second sub-section (2.1.1.2 and 2.1.1.1) – uncertainty description

- * Outline the development of the measurement uncertainty budget for CryoSat-2/SMOS
- * Discuss relative contributions of each input parameter
- * Explain why this leads to complementary uncertainty compared with the other sensor

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* Currently the authors use statistical uncertainties only for CryoSat-2 to highlight the complementary nature of the two measurement techniques. This is not sufficient. That CryoSat-2 measurements are unreliable over thinner ice, where they are associated with larger uncertainties, forms the major justification of this work. Therefore, the authors need to consider the spatial variations in the actual measurement uncertainty (not just statistical uncertainty) and show that they are larger over thinner ice. Are such uncertainty maps available? If not, can the authors produce them?

Then Figure 1 (measurement error not statistical error) and Figure 2 could be combined, and the reader has all the justification for the work in one place.

Specific comments:

P1 L9: The authors and most readers will know that “narrow-swath altimeter” refers to CryoSat-2 data. However, this is not explained in the manuscript so should not be included here. Just refer to “radar altimeter data” or similar.

P1 L17: “Essential climate variable” is somewhat an ESA tag-phrase and adds nothing to the sentence. Suggest simply changing to “Sea ice affects many climate related processes. . .”

P2 L3-4: The authors should quantify what is meant by large uncertainties over thin ice regimes, although with improved manuscript structure they might refer straight to uncertainty maps. They reference a paper by Wingham et al (2006), which I don't feel adequately supports their statement. Indeed, the Wingham paper highlights the insensitivity of the ERS altimeters to thin sea ice but the actual discussion of errors only mentions a thickness dependent error when considering observation probability, and states that the magnitude and scale of that error are not easy to estimate before improved resolution measurements from SIRAL.

P2 L13: Define statistical uncertainty, formulaically. This is also relevant for P9 L4 and P10 L8 and is needed to justify the use of gridded data as observations.

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P6 L5: Please briefly justify why SAR and SIN data need to be processed separately, and how

P6 L14: Reference is needed for the theory behind echo separation

P6 L21: What is the modified Warren snow climatology? A clearer explanation is needed of how the authors modify the climatology.

P6 L24: What is meant by the “domain of the W99 climatology”? Despite only being constrained by in situ measurements in the central Arctic, the climatology extends to all Arctic latitudes. This needs to be specifically stated.

P7 L9-10: The final two sentences are far too vague. How is the intensity “almost” independent of incidence angle? This paragraph would benefit from further explanation of the angular dependence of brightness temperature intensity over sea ice.

P8 L4-9: The justification for merging SMOS and Cryosat-2 thicknesses relies on the complementary nature of their uncertainties. Therefore, this paragraph needs expanding. In particular, how can a 100% ice coverage assumption cause underestimation of ice thickness if the condition is not met, and by how much?

P8 L20: How do OSI SAF define ambiguous?

P9 L11: How does OI minimize the total error of observations? This is a key justification for using the OI method to merge the thickness datasets, so needs further explanation.

P10 L10-11: The assumption that ice thicknesses remain static through a week is highly simplified and unlikely. Whilst I appreciate the need to make such assumptions, the authors need to be more transparent about the unlikelihood of this, or provide reference to argue otherwise.

Section 2.3.1: The development of the background field is the key aspect of the method that I am uncertain about. To ensure sufficient coverage, the authors create a background field from CryoSat-2 data extending two weeks before and after the target week.

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They do not deem this necessary for SMOS data due to its improved coverage, so only use data from the previous week. What concerns me here is the lack of consistency in the background field time-frames, and the bias it may introduce in the final, interpolated thickness product. Indeed, the authors admit that their cross validation is impacted by the fact that the SMOS background is out of phase with observations (P19 L5) and I'm unconvinced by the authors claim that the negative bias should not affect the CS2SMOS sea ice thickness retrieval. Why do the authors not create temporally complementary CryoSat-2 and SMOS background fields? Have they investigated the impact on the merged product and its evaluation with airborne data of extending the SMOS background field?

Section 3.2: What week/month/year is the cross validation carried out for? Why? The date also needs to be stated in the caption for Figure 12.

P21 L5: It would be interesting to know what fraction of AEM measurements exceed 5 m. It's not possible to tell from Figure 13 as colorbars are capped at 4 m and scatter-plots at 5 m, but just stating in the text would be sufficient.

Table 1: Incorrectly states that CS2 (monthly) coverage is Arctic-wide, as due to climatology constraints, measurements are not produced over the Hudson and Baffin Bays. This needs correcting.

Technical comments:

P1 L1: "Sea-ice thickness on **a** global scale..."

P2 L16: Should read "Besides the different sensitivities"

P7 L16: "data as **a** boundary condition"

P9 L8: **modified** climatological snow depth?

P22 L33: "r = 0.65-0.73" and "r = -0.35" i.e. spaces before and after equals sign

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Interactive comment on The Cryosphere Discuss., doi:10.5194/tc-2017-4, 2017.

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